Comment on *Cognettia* Nielsen & Christensen, 1959 (Annelida, Oligochaeta, ENCHYTRAEIDAE): giving precedence to the name promotes stability

(Case 3689; see BZN **72**: 186–192, 303–307)

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(Rut Collado, 2nd author of Case 3689, passed away 14 November 2015.)

1. The authors of the study (Martinsson et al., 2015a) that prompted our application to give precedence of *Cognettia* over its senior synonyms *Chamaedrilus* and *Euenchytraeus* (Schmelz et al., 2015) have recently defended their position to maintain *Cognettia* as an invalid junior synonym (Rota et al., 2015). We distinguish two arguments in their well-written comment: (1) Precedence of *Cognettia* will fail to achieve its main goal, to avoid confusion, because ambiguities in the meaning of species names will persist; these ambiguities and the resulting confusion can be avoided in the future if *Chamaedrilus* is maintained as valid name. (2) The change from *Cognettia* to *Chamaedrilus* has already started to be accepted; instability would result from the reversal of this situation by revalidating *Cognettia*. The arguments are framed by an elaborate presentation of the achievements and novel aspects of the scientific results in the works of Martinsson and co-authors, and emphasis is placed on the advantages of the new concepts for taxonomists and ecologists. We believe that the arguments in Rota et al. are untenable, as will be explained in the following.

2. The first argument may be paraphrased as follows: The two most-cited *Cognettia* species, *sphagnetorum* and *glandulosa*, consist, following the results of Martinsson and co-authors (Martinsson et al., 2015a,b), of four and two different species, respectively, all with different ecology. Citations of these names previous to the mentioned studies are therefore ambiguous, especially in the bulk of ecological literature, because 'one cannot tell which single taxonomic unit was the object of each ecological study, or where and when a mixture of species was involved' (Rota et al., 2015, p. 304). (The ambiguity is also virulent in the taxonomic literature, but the authors put stress on ecology because the majority of papers citing *Cognettia* as a valid name are ecological, and ecologists are the principal beneficiaries of our application.) The above-mentioned ambiguity and resulting confusion in the meaning of species names will persist regardless whether *Cognettia* is preserved or not, because the identity of the species in the previous papers cannot be reconstructed. That is, preservation of *Cognettia* is useless. On the other hand, a name change from *Cognettia* to *Chamaedrilus* will hopefully mark a 'fresh start in the ecological characterization of the individual taxonomic units in the '*Cognettia*' world' (Rota et al., 2015, p. 305), because, and this is our interpretation, it will draw attention and promote adherence to the results of Martinsson et al. (2015a, b) and so prevent ambiguity

at least in the future. In the authors' words: ' . . . stability must not have priority over lack of ambiguity' (Rota et al., 2015, p. 306).

3. This argument mixes genus level with species level and nomenclatural issues with taxonomic concepts and opinions. The confusions caused by ambiguity of names that Rota et al. (2015) refer to occur at the species level and have as prerequisite the acceptance of the taxonomic results in the works of Martinsson et al. On the other hand, the confusion caused by instability of names that is the object of our application lies at the genus level and has only to do with the Rules of Nomenclature. These are two completely different sources of confusion that should not be mixed. We also believe that decisions concerning nomenclature should not depend on particular taxonomic opinions, they may be shared by a large community of researchers or not. In our application we carefully avoided including taxonomic opinions in the argument. Furthermore, ambiguity is not a concept that is regulated by the Code except when it refers to homonymy. We are therefore not sure whether the first argument has any bearing on the Case at all.

4. The second argument draws on a list of seven papers published in 2015 that have treated *Chamaedrilus* as valid name, to demonstrate that acceptance of the change in nomenclature is already under way. A closer look at the list, however, tells a different story: two papers are authored or co-authored by Martinsson, Rota, or Erséus themselves. All of the remaining five publications have taxonomists as authors or co-authors, and we believe that taxonomists adapt easily to new nomenclatural situations, a 'change back' to *Cognettia* included. This application is not written for taxonomists. Furthermore, one of us (Schmelz) was involved in all five papers, either as co-author, peer reviewer, or as journal editor, and in more than one case he drew attention to the works of Martinsson et al. (2015a), recommending a change of name from *Cognettia* to *Chamaedrilus*. Rota et al. (2015) consider it inconsistent and contradictory to accept the new nomenclature and then to write an application against it. We regret the confusion it may have caused. However, as diligent taxonomists we accepted the most recent opinion until a formally published alternative, in this case, an application to the Commission, was available. We did not notify the submission of the application because the manuscript was still under review when the above-mentioned five papers were published.

5. Rota et al. (2015) highlight the achievements of the work of Martinsson et al. and their positive consequences for future taxonomy and ecology. They have, in our view, and with all due respect towards the scientific results of this work, no direct bearing on this application. However, several problems should not be ignored: The species diagnoses in Martinsson et al. (2015a, b) are mainly based on DNA-sequencing. Only one of the six species distinguished, *Chamaedrilus chlorophilus*, can be identified unequivocally using the conventional light-microscope. For the rest, the flagship species *sphagnetorum* included, ecologists and taxonomists will need sequencing machines and additional funds to determine their thousands of specimens. This will mean a complete change of methods that may be welcomed by molecular ecologists in well-funded institutions. The bulk of poorly funded soil zoologists, however, will have to ignore the molecular distinctions and return to a modified sensu lato concept of the species (see Schmelz & Collado, 2010), or they will abandon soil ecological work at the species level altogether. We also see conceptual problems in the paradigm shift from morphology-based to molecular taxonomy in this group. For example, all six species (see above, paragraph 2) reproduce mainly by fragmentation, while the delimitations of species boundaries among molecular clones rest on the arbitrary fixation of thresholds of genetic distances. Furthermore, we have

unpublished evidence that the situation in *Cognettia*/*Chamaedrilus* is more complicated than presented in the works of Martinsson and co-authors, especially if the relation of the fragmenting clones to the sexually reproducing species is considered. Finally, some of the type designations in Martinsson et al. (2015a) are problematic: The neotype of *sphagnetorum* was selected more or less arbitrarily among the four molecular clades; and the slide with the selected lectotype of *chlorophilus* bears a later date to the one of the original material (Martinsson et al., 2015a) and is from a collection that contained 'Typen, Kotypen und Lokaltypen' (Černosvitov 1937, pp. 191–192) – the latter are reference specimens from different localities without name-bearing status.

6. To conclude, we foresee a period of instability of names at the species level, unavoidable because due to advances in research and due to conflicting taxonomic opinions. With this prospect, however, it seems all the more important to address the additional and avoidable instability at the genus level that is caused by the invalidation of *Cognettia*. The application includes two names, *Chamaedrilus* and *Euenchytraeus*, because it depends on taxonomic opinion which of the two names is to have priority over *Cognettia*, and, as stated several times above, nomenclatural issues should not depend on particular taxonomic opinions. Our argument in favour of *Cognettia* as made in the application is not repeated here; it has been supported by Graefe & Beylich (2015) who point to the use of the name *Cognettia* in the terminology of edaphic species associations that indicate decomposer community types.

7. A final remark concerns the publication date of the work of Martinsson and co-authors that prompted our application: We cite it with year 2015 while Rota et al. (2015) prefer 2014. If this is not settled it will create confusion. The paper was published online December 23, 2014, the print version is from May 4, 2015. The headline of the online version was '*Systematics and Biodiversity* (2014), 1–21', but this version is no longer available on the website of the journal. The headline of the print version, also available electronically, is '*Systematics and Biodiversity* (2015), **13(3)**: 257–277', and page numbering has been changed accordingly. To our knowledge it is common practice to cite the version with the definite issue and page number and not the online version when it comes to citing publications. However, Rota et al. obviously consider that the online publication was effective in terms of the Code, referring to the recent Amendments of the Code that introduce new rules regarding the availability of electronic publications (ICZN, 2012). One of the prerequisites, for example, is the registry of names in ZooBank, and this was done in 2014 with the names published in Martinsson et al. (2015a). However, the cited Amendment stipulates, in paragraph 8.1.2., that in order to make names available, a work ' . . . must have been produced in an edition . . . with fixed content and layout.' As demonstrated above, the layout of the 2014 version was not fixed – year, issue, and page numbering are different now – therefore one may doubt whether the online version is an effective publication in the meaning of the Code. The Zoological Records have the 2015 version as bibliographic information, as does the website of the journal, but here with the remark: 'Published online: 23 Dec 2014'. The problem is apparently with the Commission, who should specify better the conditions that make electronic publications effective (Dubois et al., 2015). What should be avoided, however, is to cite papers with issue and page number of the print version and to maintain the year of the online version, as has been done in ZooBank with the reference in question (http://zoobank.org/Search?search_term=chamaedrilus, accessed February 23, 2016) and also in the reference list of Martinsson et al. (2015b). This creates an imaginary reference – even though the

DOI allows one to identify the source – because in 2014 there is no issue 13 of the journal *Systematics and Biodiversity*. To avoid confusion, the publication, when cited with issue and page number, should therefore go with the year 2015, in both text and references sections. In case that the online version is effective, zoological names should be cited in the text using '2014', and the reference should receive an emendation like, for example, 'effectively published 2014'.

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